AFFIDAVIT OF RICHARD T. LAHEY, JR.

This confidential affidavit of Richard T. Lahey, Jr.is made in connection with the investigation currently in process at Purdue University. I, Richard T. Lahey, Jr., being first duly sworn on oath, state that if called upon as a witness, I would be competent to testify as to the following:

1. I am making this affidavit of my own personal knowledge. All of the facts contained in this affidavit are true.

2. I am on the editorial review boards of several international journals, am a member of the National Academy of Engineering (NAE) and have been awarded numerous national and international awards (*e.g.*, the ANS' Glenn Seaborg medal, the USDoE's E.O. Lawrence Memorial Award, etc.). I am also a Fellow of the American Nuclear Society (ANS) and the American Society of Mechanical Engineers (ASME). I have had broad experience in the industrial, military, and academic sectors for over forty five (45) years. My expertise is in Nuclear Engineering technology and my CV can be viewed on www.rpi.edu/~laheyr/.

3. I was a co-author with Dr. Rusi P. Taleyarkhan ("Taleyarkhan") and others of the 2002 publication in *Science* entitled, "Evidence of Nuclear Emissions During Acoustic Cavitation." I have collaborated with Taleyarkhan for many years as a member of our multidiscplinary research team in sonofusion studies. I was also a co-author of the 2006 PRL paper and supplement entitled, "Nuclear Emissions During Self-Nucleated Cavitation" (by R.P. Taleyarkhan, C. D. West, R.T.Lahey,Jr., R.I.Nigmatulin, R.C.Block and Y.Xu).

4. Offering Review Comments and Assistance for Publication of Xu et al. Around August of 2004, I received communication from Taleyarkhan informing me of the sonofusion experiments of Yiban Xu and asking my on advice on where these results should be published.

5. I see no research misconduct in offering comments and advice (when solicited) on where to submit a manuscript for publication. Moreover, in my opinion, Taleyarkhan did nothing wrong in offering such advice to Xu et al.

6. I agreed with Taleyarkhan and fellow researcher Colin West ("West") during late 2004, that a staged approach for publication of the work of Xu et al. work would be appropriate. As I recall, we recommended *Science* followed by a larger paper in PRL, and then a more nuclear industry focused journal such as *Nuclear Engineering & Design* (NED), which I had previously served as editor of.

7. If given, I do not see any serious problems nor conflicts with anyone offering advice (when solicited by Xu) on responding to comments from journal paper (e.g., PRL) referees as long as there was no influence on the technical content being presented. In fact, as I recalled, I mentioned to Taleyarkhan that difficult referee comments should be expected for Xu's paper since we had tough reviewer comments on our 2002 *Science* paper.

8. Authorship vs Acknowledgment I believe that, that when it may occur, helping to write/compose a manuscript for the purpose of improving the quality of the presentation does not qualify for co-authorship. In my view co-authorship requires substantive technical input and/or direct participation in the experimental set-up, conduct, data acquisition, data processing, data analyses and the drawing of conclusions in the specific work being prepared for publication. While co-authorship may be offered via invitation by the lead author (i.e., the corresponding author) , it is the right of the invitee to accept or decline.

9. It is standard practice in academia to give time and advice for reviewing manuscripts when requested by colleagues, both junior and senior. It is not uncommon for

internal reviewers or referees to significantly mark-up a manuscript (especially when written by fellow scientists from foreign countries where English is not their first language). These mark-ups may at times include extensive and independent evaluations and analyses of data by the internal reviewers. If errors are found they are normally pointed out and revised estimates may be provided for the author's consideration. The original manuscript's authors normally acknowledge such assistance, guidance and counsel but in no case is it mandatory to do so. Furthermore, such assistance certainly does not in my opinion qualify for expecting nor accepting co-authorship.

10. I also attest that it is common practice in industry (e.g., the General Electric Company), at DoE's national laboratories, or even at universities for manuscripts to be written (to varying extents) by professional technical writers or by mentors providing extensive mark-ups and modifications in the case of students. Indeed, the 2002 *Science* manuscript co-authored by Taleyarkhan et al. (of which I was a co-author) was first drafted by Oak Ridge National Laboratory's ("ORNL") technical writers and finally, also was modified as to the in composition of the language and presentation by *Science* magazine's editorial staff. Clearly participation in improving the language of a manuscript for publication in journals does not meet the standard for co-authorship.

11. I did not participate in any way in the conduct or reporting of the sonofusion experiments by Xu et al. (i.e.,the NED 2005 nor NURETH-11 2005 symposium paper). I personally know of no one from our original sonofusion team (*Science*, 2002; PRE 2004) that participated in the conduct of the experiments or acquisition of data reported by Xu et al.

12. Based on published public materials (*e.g.,* Purdue's 7/2005 Press Release, Xu's statements to the German Press in July, 2007, etc.) Xu has openly declared that neither

Taleyarkhan (nor anyone in our research group) played a role in the published Xu et al. experiments, nor did they influence the data or conclusions.

13. I feel that Taleyarkhan , JaeSeon Cho and others are appropriately acknowledged in the Xu et al. NED paper and, as previously discussed, I do not believe it was appropriate to include Taleyarkhan or Cho as co-authors of this paper.

14. I understand that Rensselaer Polytechnic Institute (RPI) faculty, my colleague Robert C. Block ("Block"), has spoken at length with E. Forringer ("Forringer") of LeTourneau University in relation to his reported manuscripts (co-authored by Forringer and his students) that have been presented at two international conferences during November 2006. Forringer et al. have appropriately acknowledged the assistance provided by Taleyarkhan and Xu. The fact that Taleyarkhan and Xu may have offered assistance for the conduct of these experiments is in my view not a problem since the experiments, data gathering , analyses and conclusions were under the independent control of Forringer et al., as has been confirmed by Forringer to Block. The manuscripts and presentations by Forringer et al. during November 2006 are similar in nature to the publication of Xu et al. (e.g., NED,2005) concerning appropriate authorship.

15. Standards of co-authorship and acknowledgment are not cast in stone. The 2006 manuscripts published by the groups led by Seth Putterman and Lefteri Tsoukalas both failed to even acknowledge Taleyarkhan et al. for assistance (which was given). This is unusual in the scientific world, and it is quite peculiar since these same individuals have publicly alleged wrongdoing by Taleyarkhan et al. for their assistance to Xu et al. Any such charges by Putterman et al. and Tsoukalas et al. are, therefore in my view, disingenuous.

16. Statement on independence made in 1/06 PRL manuscript The only statement made in the joint January 2006 PRL manuscript (of which I was a co-author with Taleyarkhan

and others) was, "These observations have now been independently confirmed." This is considered appropriate for the following reasons:

17. Compared with what we (Taleyarkhan et al.) did at ORNL, Xu et al. performed their experiments in a totally different experimental configuration, the experiments were performed at a different laboratory in a different state and institution, radically different methods for nucleation were used (i.e., Xu et al. used randomly emitted neutrons of various energies from an isotope source versus the use of a microsecond duration pulse of monoenergetic neutrons from an accelerator at ORNL), different test chambers were used, different detection systems were used, calibrations were performed independently, their data was obtained separately. Moreover , it is my understanding that they derived their own observations and conclusions without the participation or influence from anyone on the original sonofusion team (Taleyarkhan et al.). Therefore, I believe that the use of the stated language in PRL is appropriate.

18. The Xu et al. NED (2005) paper was already published and Purdue's Press Release of 7/2005 mentions the levels of participation by the various entities involved.

19. None of the co-authors (including Xu himself) nor the referees or editorial staff of PRL saw anything wrong with nor challenged the statement on independent observations as documented. It was written, reviewed and agreed upon in a forthright manner.

20. MST article I was a co-author of a publication by R.P. Taleyarkhan, R.T. Lahey, Jr., R.I. Nigmatulin, "Bubble Fusion Nuclear Technology," Vol-17 *Multiphase Science and Technology* pp. 191-224 (2005) (hereinafter, "MST article"). The MST article was peer reviewed and invited as a keynote lecture at the Japan/US Seminar on Two-Phase Flow Dynamics in Japan, 2004. Based on the peer review process this paper was accepted for publication in the MST journal as written.

21. The contributions from several collaborators were acknowledged, including those from Xu et al. (whose June 2005 NED manuscript was not published at that time nor even contemplated from what I know).

22. The use and citation of figures (previously used in some of the author's prior publications) was done uniformly in other MST manuscripts in the special issues of MST devoted to the Japan/US Seminar , since most of these invited papers were review papers.

23. To remove any semblance of impropriety, a letter of clarification specifically citing the source of each figure and also directly citing the Xu et al. NED manuscript for Fig. 8b, was transmitted to the Editor-in-Chief of MST. This demonstrates responsiveness on part of the authors involved (in particular Taleyarkhan) to any concerns that wee raised.

24. I, along with Professor Serizawa (U. Kyoto), was responsible for arranging and handling the peer review process for all symposium papers that appeared in MST.

25. I am aware of the allegation against Taleyarkhan, that somehow the MST article consists of self-plagiarization. This allegation is completely unfounded and his actions in this regard certainly do not constitute research misconduct.

26. I am aware of some similarities between our MST article text and that of text contained in some of our prior publications. There was never any intent to mislead the public or MST in preparing the MST article. We never tried to show the public experiments from one source and claim them as many. Rather this was an invited review paper that summarized the state-of-the-art in sonofusion technology so the use of prior materials is expected.

27. There was never any plagiarism. If anything, honest oversights or omissions occurred which were promptly addressed by the authors when concerns were raised. More importantly, there was never any intent to somehow mislead the public.

28. Some similarities exist between the figures and text of the article in MST and the 2004 Phys. Rev. E, 69 article, but this does not rise to self-plagiarism for an untoward purpose. Rather, since this was a review paper, the similarities are merely due to our building upon past writeups,the recounting of past experiments, and the use of some of our figures in the public domain to provide a full picture of the state of sonofusion studies. In particular, there was no intent to mislead, plagiarize, or commit any type of research misconduct.

29. Acknowledgment of sponsorship Acknowledgment of sponsors in our January 2006 PRL publication has been performed in-line with expectations. It has been publicly alleged that DARPA should have been acknowledged but this allegation has no merit. To my knowledge, DARPA funding was not available during 2003 to mid-2005 when the experiments reported in January 2006 PRL were conducted. Several co-authors, including myself, worked alongside with Taleyarkhan at our discretion and without external funding (academic freedom widely permitted to faculty at US universities) to further the science of sonofusion and to answer the last remaining technical question from our detractors (*i.e.,* to be able to produce evidence of D-D fusion neutrons without the use of an external source of neutrons). One of the co-authors, West, was already retired from ORNL during that time frame, and I am aware that it is common practice for scientists at universities and even at national laboratories to use part of their personal time to pursue scholarly work beyond their normal routine duties. From evidence produced by Taleyarkhan, the gap in funding from DARPA was filled by DoE specifically for advancing the technology of this work (the federal funding from DoE was not recognized by Taleyarkhan with DoE's permission – based on email evidence provided by Taleyarkhan).

30. Taleyarkhan has provided details of work that Taleyarkhan et al. performed at Purdue with the newly obtained funds from DARPA (via UCLA) after a two (plus) year gap

during mid-2005. The tabulated list of tasks conducted by Taleyarkhan et al. and the publicized funds utilized (~$180K) at Purdue and about $70K at ORNL for similar purposes are more than reasonable but could be considered inadequate for expenditures between two institutions (Purdue and ORNL). The work for DARPA-UCLA project was identical to what our group had already published in 2002 (*Science*) which took several years and far greater resources. For one knowledgeable in the field, this research is radically different from that conducted for the January 2006 PRL studies for which our first draft was already being prepared for transmittal to journals by the time the DARPA-UCLA funds were put in place in mid-2005. To the best of my recollection, I received from Taleyarkhan summary documentation of successful sonofusion results in June, 2005. How in the world could someone surmise that DARPA-UCLA funds, which arrived into the Purdue financial system in June, 2005, actually contribute to the intense underlying research to find a way to self-nucleate in a totally different fluid-mixture of vastly different properties, in a differently designed test cell, with random vs timed nucleation, conduct a large array of tests, including careful control experiments, within "seconds" of receiving funding from a new source? The mere thought of suggesting the use of such funds for something already largely accomplished (and only requiring publication) is totally bizarre.

31. It would be inappropriate to include acknowledgment for DARPA as having supported the research leading to the January 2006 PRL publication, when in fact it was sponsored from other sources (i.e., DoE, internal university funds and personal effort). It is unusual, to say the least, that DARPA itself has not voiced concern; indeed only our well-known detractors have done so. Nobel Laureate Brian Josephson has aptly cited this allegation as, "Putterman's Flawed Case." Just because a sponsor had provided support in the past does not entitle it to be recognized in the future for research accomplishments that it did not provide

funding for. In contrast, where DARPA did indeed provide funding, our group has gratefully acknowledged their support. Thus this allegation concerning funding is completely misplaced and without merit.

32. G60 Data on sonofusion While I am not a neutron detection expert, from reviewing the evidence and speaking with my recognized expert colleague, Professor Block, the G60 data obtained at Purdue on 9/19/03 for neutron-gamma emissions during bubble fusion experiments with deuterated benzene mixtures (nucleated with an external Pu-Be neutron source) appear to be in-line with expectations upon consideration of the interference from the electronic components present in the laboratory at the time. Such interference issues should logically cancel themselves out when subtracting data taken for cavitation-on from cavitation-off conditions. The difference in these data are in-line with expectations of D-D fusion neutron emission (*i.e.*, Block has co-authored a detailed response with Taleyarkhan on this subject and they stand by the statements made therein).

33. Experts at Rensselaer Polytechnic Institute ("RPI"), principally Block, have consulted extensively in person with Taleyarkhan et al. for the set-up of LS detector data acquisition trains involving well established techniques and state-of-the-art components from reputable suppliers such as Ortec and Canberra. Indeed, Block (who in the past was Taleyarkhan's professor in nuclear engineering physics related studies at RPI) himself worked directly with and tutored Taleyarkhan et al. at ORNL to specifically set-up the detector system trains which produced the data presented in our group's various manuscripts. It is important to recognize that such a pulse-shape discrimination based detection system is not routinely included in nuclear detection teaching courses at universities that I am aware of (I served as Chairman of

RPI's Nuclear Engineering & Science department for many years). These systems require expensive detectors and various detector train components including MCAs and PCs, etc.

34. It is my understanding (per discussions with Block and Taleyarkhan) that a similar train to the one set up by Block at ORNL for our *Science* (2002) and PRE (2004) based studies was also used in the G60 laboratory. Therefore, techniques used by Taleyarkhan and Cho for detection are well-accepted in the field, with the notable exception of other electronic drive components such as amplifiers and inductor coils, etc. which evidently led to broader than expected spectra for neutron-gamma light pulse decay times. Such a spreading was also noted in the experiments of our detractors Putterman et al. (PRL, 2007). However, any such baseline effects can be reasonably expected to cancel out upon subtraction of nuclear particle signal detection between cavitation-on and off conditions at the same overall drive power level. The difference spectra (time decay of light pulses from neutron and gamma photon interactions with NE-213 molecules) clearly indicate that the neutron and gamma peaks are in the expected regions and are well separated. The neutron counts are several times that of the gamma counts as would be expected from D-D fusion neutron emission. Furthermore, the difference in pulse-height spectra also show that the neutron energies are largely below 2.5 MeV (once again, as would be expected from D-D fusion).

35. Archived date-time stamped data files from the MCA (a process which uses proprietary software) is the necessary and sufficient evidence for proving that thse data were obtained on 9/19/03 and simply could not have been fabricated.

36. In my opinion any data of the 9/19/03 type need to be accompanied with commensurate data taken under control conditions in which all parameters except one are changed (*i.e.*, use of "H" bearing liquid vs "D" bearing liquids). The specific data of 9/19/03 are

not pertinent for publication without the conduct of control experiments with non-deuterated liquids. From what I am informed, such experiments were not conducted at the time of the visit by Taleyarkhan and Cho to Purdue. Hence, the decision by Taleyarkhan et al. to not offer the scoping test data of 9/19/03 taken with deuterated benzene based mixtures is not only reasonable but sensible. Also, it is the prerogative of the persons obtaining the data to decide if and when to publish their work.

37. Communications from Martin Lopez de Bertodano (MB) on Tsoukalas group's positive results on sonofusion As of September 2003 my former PhD student, Bertodano (now on the faculty at Purdue and, at the time, part of Tsoukalas' bubble fusion group) documented the group's sustained positive results (3 sigma+) in their experiments. I was aware (as communicated to me by Bertodano) of the successes attained by the Tsoukalas group during 2003.

38. Communication from LT re: requesting review for BBC and NURETH-11 Tsoukalas (in his email letter to Colin Murray of BBC's Horizon show, which he shared with me and Taleyarkhan) himself admitted to obtaining positive results for sonofusion. It was Tsoukalas himself who requested comments from Taleyarkhan and Lahey and willingly followed through with his admissions, a full year after his group had completed their work. For some odd reason, during mid-2005 when it was time for Tsoukalas to submit his group's final manuscript for presentation to NURETH-11, their manuscript was pulled back.

39. The above mentioned issues highlighted in the Press are a either wrongful or inaccurate allegations against Taleyarkhan (and in many respects all of us who worked with him) and have no merit. These are apparently desperate attempts by our competitors to detract from our seminal work on the discovery of sonofusion.

40. Finally, I have known Taleyarkhan since 1977, ever since Taleyarkhan joined RPI as a graduate student. I have interacted with Taleyarkhan as a professor and mentor (I was his Ph.D thesis advisor) and as a colleague for close to thirty (30) years and can vouch for Taleyarkhan's integrity, professionalism, and sound research practices. He is a respected scientist, is very intelligent , hard-working, and is always honest, trustworthy and fair with the people he interacts with.

R. T. Lahey

DR. RICHARD T. LAHEY, JR.

SUBSCRIBED AND SWORN TO
before me this 30 day
of January, 2008.

Michele E. Kiernan

Notary Public

2/28/11